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CONTENTS.

The British Association for the Advancement of Science:—
Address before the Section of Mathematics and Physics: PRINCIPAL E. H. GRIFFITHS.. 353

The American Association for the Advancement of Science:—
Section E—Geology and Geography: DR. EDMUND OTIS HOVEY..... 365

Scientific Books:—
Zsigmondy's 'Zur Erkenntnis der Kolloide: PROFESSOR S. LAWRENCE BIGELOW..... 372

Scientific Journals and Articles..... 374

Discussion and Correspondence:—
When did Franklin invent the Lightning-rod?: A. LAWRENCE ROTCH. *Dried Cotton Cultures once more:* DR. GEORGE T. MOORE. 374

Special Articles:—
Geology of South Brazil: PROFESSOR I. C. WHITE 377

The Forest Districts of Uganda..... 379

Appointments and Removals at Stanford University 380

Scientific Notes and News..... 381

University and Educational News..... 384

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THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

ADDRESS BEFORE THE SECTION OF MATHEMATICS AND PHYSICS.

MY predecessors in this chair have in general been able to make communications to the section conveying the results of investigations of their own, or enunciating some principle which would throw a fresh light on the discoveries of others. Mine is a far less happy lot. During the past four years and a half I have been engaged in administrative duties of such a nature that no time has been available for personal scientific work, and little energy even for the study of the work of others. In these circumstances it might have seemed more fitting if I had refused the honor which the council of the British Association conferred upon me by the request that I would undertake the arduous duties which fall to the lot of the president of Section A. Nevertheless, after much hesitation, I decided to accept the invitation, in the hope that as a looker-on at the struggle of others, and with the experience of an old participator in the fray, I might be able to communicate some impressions which had possibly escaped the notice of those whose attention was necessarily more directed to some special branch of inquiry.

I trust that these words of apology may to some extent explain the nature of what must appear a fragmentary discourse.

In the interval which has elapsed since the last meeting of the association we have lost many men whose names were household

words within the walls of the physical laboratory. It is here only possible briefly to refer to the labors of a few of those distinguished seekers after natural knowledge.

The work of Dr. Sprengel has been by no means an unimportant factor in the advance of our knowledge of radiant energy, X-rays, etc., if only on account of the perfection of the apparatus for obtaining high vacua which will ever be associated with his name. The practical effect of his discoveries was considerable, for the business of electric lighting is undoubtedly greatly indebted to his labors. Born in 1834, he settled in England at the age of twenty-five. He was elected a fellow of the Royal Society in 1878, and resided in this country during the remaining years of his life.

The death of Charles Jasper Joly, F.R.S., at the early age of forty-one, robbed mathematics and astronomy of one of their most devoted disciples. His 'Manual of Quaternions' is well known, and those acquainted with his astronomical work are confident that, had his life been spared, he would, as astronomer royal of Ireland, have added luster to an office held by many distinguished predecessors.

Samuel Pierpont Langley was born in 1834. In 1866 he became director of the Allegheny Observatory at Pittsburg. His first work was the institution of a uniform system of time from the Atlantic seaboard to the Great Lakes. This, the first successful attempt to introduce uniformity of time over a large area, was subsequently widely imitated. In 1880 he invented the bolometer, and thus opened out a large new field of investigation into the invisible rays of long wave-length proceeding from heated bodies. He analyzed in minute detail the lunar heat spectrum, and, more recently, he conducted an inquiry into the nature of the radiations emitted by the glow-worm. In 1881 he conducted his researches into the solar heat of the earth's atmosphere. In 1887 he became secretary to the Smithsonian Institution. The result of twenty years' labor is to be found in the accurate determination, by temperature alone, of more than seven hundred lines in the invisible red spectrum, lines which are fixed with an average probable error of about one second of arc. In 1891 he published his experiments in aerodynamics, in 1893 'The Internal Work of the Wind' and in 1896 he demonstrated by actual experiment that a body nearly a thousand times heavier than air can be driven through and sustained by it. His published works show great literary charm. He especially excelled in the presentation of abstruse subjects in simple and non-technical language. This is, perhaps, hardly the occasion to refer to his social qualities. Those who had the privilege of his acquaintance, however, can best testify to his quickness of insight, his intense sympathy, especially with the young, and the impression of capability which he produced upon all with whom he came in contact.

The tragic death of Professor Curie was felt as a calamity, not only by those closely interested in the march of scientific discovery, but also by those who had but a superficial knowledge of his work. A teacher for more than twenty years, he was, nevertheless, enabled by his enthusiasm and energy to perform those researches which will ever be connected with his name and that of his wife. So entirely has public attention been attracted to their joint work on the separation of the compounds of radium and their properties that we are apt to overlook other great services he rendered to science. His paper on 'The Effect of Temperature on the Magnetic Properties of Bodies' led to the discovery of the

law that for feebly magnetic substances the coefficient of magnetism varies inversely as the absolute temperature. He also pointed out that the magnetization of diamagnetic substances appeared to be independent of the temperature and physical state, indicating diamagnetism as an atomic property.

It is pleasing to reflect that the importance of his discoveries received immediate recognition. It was but three years before his death that he announced to the French Academy the discovery of the new element, and in the same year he and Mme. Curie received the Davy medal of the Royal Society and the Nobel prize; and in July of last year he was elected to the French Académie des Sciences. He was one of the most modest and retiring of men, and this honor came to him unsought; his name will ever be remembered as one of the most notable of that brilliant band of workers who have within recent years so greatly extended the domain of physics by the discovery radioactivity.

A quarter of a century has passed since this section, meeting in this city of York, had the privilege of listening to a presidential address by the pioneer of natural knowledge whom we now know as Lord Kelvin, and it may possibly be a not unprofitable task to review briefly a few of the advances which must render the interval a memorable one in the annals of science. Lord Kelvin summarized the stores of energy from which mechanical effects can be drawn by man as follows: (1) the food of animals, (2) natural heat, (3) solid matter found in elevated positions, (4) the natural motions of water and air, (5) natural combustibles, (6) artificial combustibles.

The twenty-five years which have since elapsed have not made it possible to extend this list. It is true that within the last few years the discoveries connected with radioactivity have enormously increased our estimate of the stores of energy surrounding us, but so far these additional stores can not be regarded by us as stores from which 'mechanical effects may be drawn by man.' It is possible that in the ingenious radium clock which we owe to Mr. Strutt we have a source of mechanical energy unsuspected in 1881, but, at all events, regarded from a commercial standpoint, it can hardly be considered as 'available by man.' Nevertheless, there is a sense in which it may be said that we are profiting by atomic energy, for we are no longer bound to limit our estimate of the energy due to the radiant heat of the sun and the internal heat of the earth by previously known dynamical considerations, and, in consequence, our opinions with regard to the limit of the ages which the physicist could allot to the evolutionist have undergone profound modification.

I here wish to draw attention to some of the conclusions to which we are led by the work of Mr. Strutt.

Assuming the earth to be in thermal equilibrium, then, even if the whole of this interior heat be due to radium alone, the mean quantity per cubic centimeter can not much exceed 1.75×10^{-13} gram. The conclusions of Rutherford, based on somewhat different values for the constants involved, give an equivalent of 1.52×10^{-13}. Now Strutt has found that the poorest igneous rock examined by him, namely, Greenland basalt, contains more than ten times this quantity, and an average rock fifty or sixty times the amount. The assumption that the earth is cooling only aggravates the difficulty, and facts appear to tell against the theory that it is getting hotter. Also, we must take into consideration the heat due to the existence of uranium, thorium, etc.

We appear, therefore, to be driven to one of two assumptions: either (*a*) that the rate of heat production by radium diminishes as we approach the center of the earth; or (*b*) that the interior of the earth differs markedly in constitution from the exterior crust.

It is true that Mr. Makower has shown that there is a slight change of activity in one of the radium products about the temperature of 1,200° C., and it is very desirable that this inquiry should be pushed to much higher limits. At the same time, it appears evident that but a very slight change in activity takes place at temperatures below 1,500° C.

Now Mr. Strutt has shown, arguing from known data, that the maximum temperature at the bottom of a crust of about forty-five miles in thickness, must be in the neighborhood of 1,530° C., although some amount of uncertainty is necessarily induced by our want of knowledge of the conductivity of rock at high temperatures. Anyhow, it is probable that at the depth indicated the temperature does not exceed the melting-point of platinum. Such a crust would contain about one thirtieth of the earth's volume, and if throughout it the radium heat energy were of the average of that exhibited by many samples examined by Strutt, the temperature of the earth could be maintained until our stores of uranium suffered sensible depletion. Such an assumption would lead to the conclusion that the whole of the central portion of the earth consists of non-radioactive substances at an approximate uniform temperature somewhat below the melting-point of platinum. A brief summary of the evidence previously at our disposal may not be out of place.

In the first edition (1867) of Thomas and Tait's 'Natural Philosophy' we find the tidal evidence summarized as follows:

> It seems certain, therefore, that the tidal effective rigidity of the earth must be greater than that of glass.

In the 1883 edition of the same work a discussion of the question by Professor George Darwin is given. He states:

> On the whole we may fairly conclude, whilst there is some evidence of a tidal yielding of the earth's mass, that yielding is certainly small, and that the effective rigidity is at least as great as steel.

In a later paper (*Proc. Roy. Soc.*, 1885) Darwin pointed out that this conclusion was based on the assumption that oceanic tides would have their equilibrium value, and that the validity of this assumption was open to doubt. Nevertheless, the evidence clearly indicated a high degree of effective rigidity.

Hough (*Phil. Trans.*, A, 1895, 1896) discussed the variation of latitude, and, after correcting a small mistake of Newcomb's (who was the first to suggest the explanation), found the prolongation of the Eulerian nutation from 305 to 430 days as indicating an effective rigidity of the earth about equal to that of steel. Wiechert (*Trans. Roy. Soc. Göttingen*, 1897), of Göttingen, found that the mean density, ellipticity and precessional constant were consistent with the hypothesis of homogeneous core with lighter surface layer.

Mr. R. D. Oldham (*Phil. Trans.*, 1900), in a paper on the 'Propagation of Earthquake Waves,' came to the conclusion that the evidence pointed to a central metallic core, and to the existence of marked differences in the physical constants of the core and the surrounding crust. He, however, assigned a comparatively small radius to this core, viz., about 0.55 that of the earth.

I will now call your attention to the light thrown on this subject by the recent investigations of Professor Milne. The difference in the rate of propagation of earthquake waves through the earth's interior

and through the crust has led him to the conclusion that the material below a depth approximating to thirty miles is of a uniform nature, and that the change in physical constitution is abrupt at some such depth as that indicated. He writes as follows:

> For chords which lie within a depth of thirty miles the recorded speeds do not exceed those which we should expect for waves of compression in rocky material. This, therefore, is a maximum depth at which we should look for materials having similar physical properties to those we see on the earth's surface; beneath this limit the materials of the outer part of this planet appear rapidly to merge into a fairly homogeneous nucleus with a high rigidity.

In the *Transactions of the Royal Society* for 1905 will be found a paper by Lieut.-Colonel S. G. Burrard on 'The Intensity of the Force of Gravity in India.' Colonel Burrard writes as follows:

> Geodetical observations have shown that the density of the earth's crust is variable, but they have not given any positive indications of the depths to which these observed variations extend. All calculations of the depths of subterranean variations in density and of the mountain compensation have, therefore, to be based on arbitrary assumptions of depth. The fact that the plumb-line seems generally to respond readily to the results given by the pendulum perhaps justifies the inference that the observed variations in the density of the earth's crust are not deep-seated. If an abnormal amount of matter exists in the crust near the surface, it will exercise direct effects upon plumb-lines and pendulums in the vicinity, but if it lies at a great depth its effects, especially on plumb-lines, will be less perceptible. * * * I have taken several instances of abnormal pendulum results from table, and have found in each case direct response from the plumb-lines at neighboring stations. This conformity could hardly ensue if the variations in density extended to greater depths than thirty or forty miles. Our results do not justify us in asserting that no deep-seated variations in density exist, but they do justify the belief that the variations in density which have been discovered are apparently superficial.

It is interesting to notice the agreement between results drawn from such dissimilar sources. On the one hand we have had to deal with effects produced by almost inconceivably small particles traveling with immense velocity; on the other, with effects dependent upon the behavior of 'the huge terrestrial globe.' That travelers starting from such opposite extremes should arrive at a common destination is in itself a striking example of the scope and accuracy of the work undertaken by investigators in physical science.

It is possible that the evidence from each source, considered independently, might be regarded as inadequate, but the cumulative effect is sufficiently strong to justify the belief that some marked physical change in the constitution occurs at a depth of some thirty to fifty miles.

At all events, we have indications that, with the exception of a comparatively thin crust, the earth consists of a non-radioactive substance with a rigidity approaching that of steel, with an average temperature in the neighborhood of 1,500° C., and a density at that temperature of about 5.6° C.

An interesting question awaiting solution is the probable constitution of this core.

The above is but an example of the many fascinating problems upon which fresh light has been thrown by the revelations of recent discoveries in radioactivity, and the temptation to dwell on such themes is correspondingly great: but I feel that such a task should be committed to hands more capable than mine.

Fortunately, in the discussions which will take place during our meeting ample opportunity will be afforded those entitled to speak with authority. Nevertheless, there are one or two further aspects of the matter which I will venture to touch upon, although but an onlooker. I would, first of all, urge the importance of a study of

what may be termed the natural history of the elements. We require more information as to their comparative proportions in different localities. The fact that, given the amount of uranium in a sample of native rock, we can predict with certainty the amount of radium contained in the same specimen is of startling significance.

The natural law which governs the proportions of these two substances may have a far wider-reaching scope than we at present suspect. Nature appears to present to us a grouping which would not naturally have occurred to the mind of the chemist; lead and silver, copper and gold, and, again, platinum and iridium seem invariably to be introduced to us by nature as if bearing to each other some kind of blood relationship.

The facts we already possess seem dimly to indicate some close relation between elements which we have hitherto considered as outside the bounds of consanguinity, and for a fuller knowledge of this important branch of natural history we require the assistance of the practical engineer, the geologist, the metallurgist and the chemist.

Many of the results arrived at by the investigators into the phenomena of radioactivity can apparently only be verified by the lapse of considerable intervals of time. It is, probable, for example, that we can estimate with some degree of accuracy the time required for the dissolution of half a given mass of uranium or radium, but the complete verification of our inferences must probably be left to a future generation. If we accept this view, it is our duty to provide our successors with data on which their conclusions may be based. If, for example, carefully determined masses of the more radioactive substances could be placed in such circumstances as to remain untouched until the meeting of this association some hundred years hence, our successors, who would doubtless be equipped with apparatus of research more accurate and more sensitive than any in our possession, would at all events be placed in a position to establish by direct methods the accuracy of inferences based upon the experimental data now at our disposal. This task is one which, it appears to me, might well be undertaken by Section A, and I trust that this suggestion may be held worthy of some consideration.

It appears probable that one gram of radium diminishes in weight by about half a milligram per annum; hence, if the funds of this society admitted of the imprisonment of some definite mass of radium, our successors a hundred years hence would, even if they possessed only the apparatus now at our disposal, be able to determine its loss with sufficient accuracy to enable them to verify the truth of the conclusions arrived at by the physicist of to-day, while the investigation of the radioactivity of the residue would possibly throw light on many problems now awaiting solution.

It would appear that if we made a similar imprisonment of uranium, a like degree of accuracy would not be attainable until after the lapse of half a million years, and I am afraid that our interest in the work of our successors can not be expected to cover so long a period. Nevertheless, it is probable that the presence of the products of decomposition could easily be detected after the lapse of a comparatively short interval of time.

The experiment might well be extended so as to include examples of all the elements capable of such treatment; and with each prisoner should be placed a full record of its physical constants, such as mass, density, electrical conductivity, specific heat, etc., with a clear indication of what is believed to be the probable accuracy of such determination.

During the past twenty-five years much thought has been devoted to the accurate determination of certain physical constants. This is very apparent in the case of one of the most important—namely, that commonly termed the 'mechanical equivalent of heat,' or, as I prefer to define it, the 'thermal equivalent of energy.' When Lord Kelvin addressed you in 1881, I think it probable that he would have indicated the value obtained by Joule—viz., 772.6 foot-pounds—at Manchester, as the quantity of work required to raise the temperature of one pound of water through 1° F. at 62° F. It is true that the results of Rowland's classical investigation were published in 1880 and 1881, but the discrepancy between his conclusions and those of Regnault regarding the change in the specific heat of water at temperatures between 0° C. and 30° C. introduced an element of uncertainty.

As a consequence of this discrepancy much experimental work on the subject has been performed in the last quarter of a century, and I think it may be said without hesitation that the value of this important constant is now ascertained with an accuracy of about one part in 2,000. The amount of labor which has been employed in the determination of this thermal constant is extraordinary, and, as I have pointed out elsewhere, it well illustrates the cosmopolitan character of scientific investigation.

I have given reasons (Griffiths, 'The Thermal Measurement of Energy') for specially selecting for consideration the determinations of Rowland, of Bartoli and Stracciati, of Ludin, of Callendar and Barnes, of Schuster and Gannon, and I have ventured to add my own. Thus Baltimore, Pisa, Zurich, Montreal, Manchester and Cambridge have all contributed to the solution of the problem, and we may now with some certainty say that 777.7 foot-pounds at Greenwich are very closely the equivalent of the amount of heat required to raise one pound of water through 1° on the hydrogen scale at 63.5° F.

It may possibly appear that the result just quoted is a somewhat poor return for the expenditure of so much thought and labor. I would call attention, therefore, to the fact that the value of this equivalent is dependent on the measurements of many other natural constants; hence any agreement between the results obtained by the observations of Rowland and some of the other observers I have mentioned would only be possible in the absence of errors of appreciable magnitude in the determinations of mass, of change of temperature, and of electrical resistance and current. Certain discrepancies have led to the discovery of a hitherto unsuspected cause of inaccuracy, especially in the determination of temperature, and thus the inquiry has rendered valuable service in many branches of physical inquiry.

For example, so far back as 1893 I ventured upon a prophecy that the value assigned to the E.M.F. of a Clarke's cell was somewhat too high, and that it was possible that 1.4328 represents more truly the potential difference of a Clarke's cell at 15° C. than the ordinarily accepted value of 1.4342. In the report of the Electrical Standards Committee for 1897 will be found a discussion of this matter, and one of the consequences of the deliberations of that committee is to be seen in the ampere balance now standing in the National Physical Laboratory.

The results of the observations obtained by this instrument will, I believe, shortly be published by Professor Ayrton and Mr. Mather, but I am at liberty to state that, so far as the observations have been reduced, they point to the conclusion that

the prophecy to which I have referred is closely fulfilled. We may say, therefore, with some confidence that the values of those units which form the basis of our system of electrical measurement are not only practically determined with a high degree of accuracy, but that also our measurements of temperature and of energy are placed on a satisfactory footing.

The last few years have been fruitful in revelations which not only profoundly affect the views of students of science, but also are of such a nature as to catch the eye of the public. In some cases the applications of these discoveries to the purposes of mankind have been evident and immediate. Every well-equipped hospital possesses apparatus for the production of Röntgen rays, and I suppose that every bluejacket in the navy has some degree of acquaintance with those applications of science which have resulted from the discovery of Hertzian waves.

The ambition of the student is naturally fired by such examples, and there is a possible danger that the plodding but absolutely necessary work of accurate measurement may suffer by neglect. I, therefore, venture to repeat the well-established axiom that our advance in scientific knowledge is a function of accurate measurement, and that the student who devotes his energy to the determination of some physical constant is probably giving a 'point of departure' to the pioneer. For it must ever be remembered that to the scientific investigator the rule of three has ceased to hold any significance.

When Lord Rayleigh discovered that the mean weight per liter under standard conditions of chemical nitrogen was 1.251, and that of atmospheric nitrogen was 1.257, the believer in the rule of three would have been unlikely to suspect that this difference of 0.006 would supply the clue which led Lord Rayleigh and Sir W. Ramsay to the discovery of a new element, a discovery which in its turn led to others of possibly even greater importance. For all we know the next decimal place in any hitherto accepted value may afford another example of the truth of the statement that a part may be greater than the whole.

At the time when Lord Kelvin delivered the address to which I have already referred, the truth of the second law of thermodynamics was probably not so generally accepted as is the case at the present time. Each apparent example of violation of that law has on closer examination proved to be additional evidence of its validity. We seem unable to find those 'sorting demons' of Maxwell's, the existence of which appears necessary for its violation.

Mr. Campbell recently expressed doubts as to the application of thermodynamic considerations to osmotics. He contended that the errors in the determination of osmotic pressure were greater than those which could be attributed to experimental sources. Now, the theoretical relation between osmotic pressure and the freezing-point is based directly on thermodynamic considerations, and it was because I entertained a belief that the most direct evidence of this much-debated matter could be obtained from the observation of the freezing-point of a very dilute solution that I embarked on a series of somewhat elaborate experiments during the years 1897 to 1901. My removal from Cambridge and the death of my assistant, Mr. C. Green, compelled me to leave that inquiry in an unfinished condition. Nevertheless, I had investigated the depression of the freezing-point in certain solutions varying in strength from 0.0003 to 0.025 gm.-molecule per liter.

Subsequently to my departure from

Cambridge Mr. Bedford reerected the apparatus in another building. After having surmounted great difficulties, he repeated many of my experiments, and he informs me that the numbers he has so far obtained are in almost entire agreement with those previously obtained by me. The molecular depression in the case of cane sugar I found to be 1.858, of potassium chloride 3.720, and I understand that Mr. Bedford's experiments agree with these results with a discrepancy of less than 1 part in 1,000. The most probable number obtained from theoretical considerations would be in the former case 1.857, in the latter 3.714. As Mr. Whetham has pointed out, unless there is some balancing of opposite errors of a very improbable nature, it is difficult to imagine a more direct vindication of the application of thermodynamic considerations to the phenomena of solution. I may add that I also examined correspondingly dilute solutions of sodium chloride, barium chloride, sulphuric acid, potassium bichromate, magnesium chloride and potassium iodide; but, owing to the circumstances to which I have referred, I was unable to repeat these experiments in such a manner as to enable me to attach great importance to the resulting figures. Nevertheless, I obtained values which strengthened the conclusions to which I was led by the more exhaustive examination of the dilute solutions of sugar and potassium chloride.

So far back as the Liverpool meeting of this association I expressed a hope that the experimental difficulties of the direct measurement of osmotic pressures would be overcome, as such direct measurement would afford the most useful data by means of which to obtain further light on the much-vexed question of the nature of solutions. I remember, also, that it was the general opinion of those who had given attention to this matter that the experimental difficulties were insuperable.

I am glad, therefore, to have this opportunity of stating my high appreciation of the manner in which Lord Berkeley and Mr. Hartley have grappled with the difficulties of this investigation. They have proved that the osmotic pressure obtained by direct measurement agrees with that derived from vapor-pressure observations to within less than 5 per cent.[1] The agreement is of great importance, as it diminishes our doubts as to the extent to which the imperfections of semi-permeable membranes may affect the validity of results dependent upon their behavior, and points to the possibility of determining the osmotic pressures of concentrated solutions by measurement of their vapor pressures.

I trust it will not be thought out of place if I here refer to the interesting correspondence which has recently appeared in *Nature* on the thermodynamic theory of osmotic pressure, and the allied, but by no means identical, problem of the difference between electrolytic and non-electrolytic solutions.

On the one side we have Professor Armstrong, whose chief desire appears to be the vindication of the moral character of what he terms 'the poor molecule'; and Mr. Campbell, whose doubts concerning the second law of thermodynamics are closely connected with a lurking belief in the existence of Maxwell's 'sorting demons'; and by way of reserves we have Professor Kahlenberg, who contends that 'thermodynamic reasoning can not be applied to actual osmotic processes' on account of the

[1] Concentration.	Direct O. P. at 0° C.	O. P. deduced from Vapor Pressure at 0° C.
540 grains per lit. solution..	67.5	69.4
660 grains per lit. solution..	100.8	101.9
750 grains per lit. solution..	133.7	136.0

Proc. Roy. Soc., June, 1906.

'selective action of the membrane' and 'insists that the formation of crystals from a solution or the concentration of a solution by evaporation are not osmotic processes.'

On the other hand we have Mr. Whetham, who, I confess, seems to me to be capable of holding his own without need of reinforcements. He has pointed out that confusion has arisen from the use of the term 'osmotic pressure' to denote the actual pressure experimentally realized in certain conditions, as well as the ideal pressure required in thermodynamic theory. With regard to the theory of electrolytic dissociation, Mr. Whetham shows that the fact that the velocities of the ions are constant in dilute solutions and decrease slowly with increasing concentration, while the conductivity of a dilute solution is at most proportional to the first power of the concentration, appears irreconcilable with any assumption as to the existence of the active part of an electrolyte in the form of combined molecules when in solution. I would here join with Mr. Whetham in the request that those who oppose the theory of ionic dissociation would state their views as to the mechanism of electrolysis, and their reasons for supposing that the application of the principles of thermodynamics to the phenomena of solution is unjustifiable.

Professor Armstrong remarks that it is unfair to 'cloak the inquiry by restricting it to thermodynamic reasoning, a favorite maneuver with the mathematically minded.' He adds that such a course may satisfy the physicist, but 'is repulsive to the chemist.'

The inquiry, 'Why is the application of thermodynamic reasoning repulsive to the chemist?' naturally suggests itself. I confess that at one time I regarded the extreme advocates of the theory of ionic dissociation with a certain amount of suspicion, but I think that most of those who have studied the evidence now at our disposal, or who have been engaged in experimental investigation into this interesting branch of physics, can not fail to agree with Mr. Whetham that, as regards the fundamental conceptions of the theory, 'the cumulative evidence seems overpowering.' At all events, we may consider that the application to the phenomena of solution of reasoning based on thermodynamic considerations is justifiable, until we are presented with stronger arguments than those based on the repulsiveness to certain chemists of the conclusions to which it leads, or the doubt it throws upon the activities of Maxwell's demons and the selective action of semi-permeable membranes.

I will now trespass upon your forbearance and pass from the consideration of such special departments of natural science as usually engage the attention of members of this section to some more general considerations, which naturally arise in any comparison of our knowledge of to-day with that which we possessed when we last met in this city.

It will, I think, generally be admitted that during the last twenty-five years the increase in our 'natural knowledge' has been greater than in any previous quarter of a century.

Day by day we are adding new facts to our storehouse of information, until it has now become impossible for the individual to have more than a superficial knowledge of the contents of the building. And although this accumulation is one which we may well regard with satisfaction, it necessarily gives rise to difficulties unfelt by our predecessors.

I venture to indicate one of such difficulties, one which has been brought home to me both by my experience as an examiner and by the fact that during the past few years I have had to preside over many

meetings of examiners, and to mark the effect of examinations on the teaching in our universities.

We now expect a student to acquire in a three years' course a far greater amount of information than was considered necessary, say, twenty-five years ago. The attention both of the teacher and of the taught is naturally directed to those extremities of the branches of science in which the growth has been most marked in recent years, and I venture to think that there is in consequence some danger of our neglecting the roots of the whole matter. Compare, for example, a final paper in chemistry in any one of our universities with its predecessor of a quarter of a century ago.

The enormous advance of organic chemistry has necessarily reacted on the examinations, and thus the student is unable to devote an adequate proportion of his time and attention to the foundations of the subject. The same remark applies in the domain of physics. There is a danger, therefore, of our educational edifice becoming top-heavy.

I have heard complaints, on the one hand, from the examiners that while the candidates frequently exhibit considerable knowledge of the most recent scientific developments, they show a lamentable ignorance of the simple phenomena and the principles they illustrate. On the other hand, I have heard from candidates that many of the questions were too simple—that they were concerned with principles and facts to which their attention had not been directed since they first began the study of natural science.

My own experience has been that the simplest questions are those answered in the least effective manner. A candidate unable to give satisfactory illustrations of Newton's laws will discourse upon the mass of an electron or the nature of the Röntgen rays, and attempt the solution of problems on such subjects as Hertzian waves and electric convection.

I hope that the attention of both examiners and teachers may be directed to the best methods of dealing with what appears to me to be not only a serious but an increasing evil.

To pass from one of the inconveniences which inevitably arise from growth, it is pleasant to dwell upon its more gratifying consequences.

Perhaps one of the most marked characteristics of the progress of science in recent times is the increasing public appreciation of the importance of original investigation and research.

The expansion of the university colleges in number and importance has greatly assisted and quickened this movement.

Twenty-five years ago there were comparatively few laboratories which held out any possibility of research to the English student. True, there were giants in those days, men, as a rule, working under difficulties greater than those encountered by their successors of to-day. The better equipment of our laboratories and the growth in the number and activity of our scientific societies have played no small part in stimulating public interest. Nevertheless, much remains to be done. Those who have read Professor Perry's somewhat pessimistic words on England's neglect of science must admit that, however rapid our progress, the British people have not yet so fully awakened to the national importance of this question as some of our competitors.

The idea that a degree is one of the chief objects of education yet lingers amongst us. The conviction that it is a national duty to seek out and, when found, utilize the latent scientific ability of the rising generation for the purpose of adding to our stores of

natural knowledge still needs to be brought home to the 'man in the street.' And here I would venture to indicate my personal belief in the necessity of more free communication between the laboratory and the market-place. It is possible that the language of science is becoming too technical, and that the difficulties with which scientific inquirers have been faced in past times have tended to habits of exclusiveness. For example, complaints are frequent that our manufacturers are less alert in grasping the practical applications of scientific discovery than their competitors in Germany and the United States. I confess, however, that it seems to me possible that the fault is not altogether on the side of the manufacturers. We want missionaries to preach the doctrine that one of the greatest of national assets is scientific discovery. If we can convince the men of business of this country that there are few more profitable investments than the encouragement of research, our difficulties in this matter will be at an end.

It is my lot to serve on the education committees of three county councils, and I have been much struck by the readiness of the members of those bodies to extend such encouragement whenever it has been possible to convince them that the results may conduce to the prosperity, the comfort and the safety of the community.

It has also been my privilege to address meetings of the men who work in the coalfields of South Wales. I have attempted to direct their attention to the advantages which they have derived from the labors of those who have endeavored to probe the secrets of nature in the laboratory; I have tried to show how discoveries based on the researches of Humphry Davy, Faraday, Joule, for example, have not only diminished the dangers to which miners are exposed, but have also, by increasing the demands upon our stores of energy, given employment to thousands of their fellow-workers.

My experiences lead me to the belief that these men are ready to support the action of their representatives in extending support and encouragement to all efforts to assist the advance of scientific discovery.

It is possible that in dwelling on this matter I am trespassing on your forbearance, but I can not resist this opportunity of pleading for the extension of your sympathies beyond the walls of the laboratory. The old toast, 'Here's to science pure and undefiled; may it never do a ha'porth of good to anybody,' may possibly be an excellent one in the laboratory; for, so far as I know, no great scientific principle has ever been established by labors prompted solely by desire for financial gain. Nevertheless, if we wish for the support of our fellow-countrymen, that toast is not one for public dinners. There is no scientific society which is brought into such close contact with the public as is the British Association, and affiliated with that association are some scores of local scientific societies, containing many thousands of enthusiastic observers and inquirers. If this great organization were seriously to take up the task of bringing home to the minds of the people of this kingdom the enormous value of the results of scientific inquiry, I believe it might be possible to change the indifference and apathy of our public bodies into active interest and encouragement. If each affiliated society would institute a series of public non-technical lectures, of such a nature as to bring home to the minds of the hearers some comprehension of the results of the work of Faraday, of Wheatstone, of Pasteur, of Maxwell, of Lister and of Kelvin, the change in the public attitude would be real, evident and fruitful.

In conclusion, one is tempted to seek for

the underlying cause of the acceleration in the rate of advance of natural knowledge. Is it to be found in the increase in power of the human intellect, or the diversion into one particular channel of activities previously otherwise employed? It is possible that the human intellect has, by the processes of evolution, become more powerful, and that man's ability to decipher the secrets of nature has thereby increased. I think, however, that it would require a bold advocate to support this thesis. If any such mental evolution has taken place, it is strange that it should be restricted to one particular sphere of activity. Are our poets and authors of to-day greater than Homer, our statesmen than Pericles? Or, passing into the domain of science, can we say with confidence that, in pure power of reasoning, Maxwell was undoubtedly the superior of Archimedes?

I have elsewhere indicated what appears to me to explain the mystery of this acceleration, namely, *the extension of our senses* by mechanical appliances. When we supplement our eyes by the bolometer and the electric coherer, the range of our vision is augmented a thousandfold. By the use of the electroscope and the galvanometer we have extended our senses of sight and touch until we can detect the presence of an electron.

Having realized the imperfection of our faculties, we have called upon nature in all departments of science to supply our deficiencies, and are thus enabled to walk with confidence where previously all seemed dark.

From the time of Archimedes to that of Bacon we despised natural knowledge while we deified intellect and authority; hence for nearly 2,000 years our record was one of retreat rather than advance. When the philosopher left his study and applied his powers of observation to the phenomena of the universe, progress became a reality, and thenceforward the march of discovery has known no backward step. We have, therefore, every reason to believe that when the association again visits this ancient city our president will be able to chronicle an increase in natural knowledge even greater than that which has been one of the distinguishing characteristics of the last quarter of a century.

E. H. GRIFFITHS.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

SPECIAL MEETING, ITHACA, NEW YORK, JUNE 28–JULY 3, 1906.

SECTION E—GEOLOGY AND GEOGRAPHY.

THE section organized at 11 A.M., Friday, June 29, directly after the adjournment of the first general session of the association, in the geological lecture room, McGraw Hall, with Vice-president and Chairman A. C. Lane in the chair and thirteen members and seven visitors present. Before proceeding with the program of papers the following preamble and resolution were offered by Mr. George H. Chadwick, of Albany, New York:

WHEREAS in the decease of Professor Israel C. Russell Section E has lost an efficient officer and one of its foremost workers and best loved members.

Resolved, That the section express its deep sorrow and its sense of the great loss to geologic science through the event.

Remarks to the motion were made by Messrs. A. C. Lane, H. S. Williams, D. S. Martin and E. O. Hovey.

The following papers were then read in accordance with the printed program:

Revision of the Geological Section passing through Ithaca, N. Y.: Professor H. S. WILLIAMS, Cornell University. (By permission of the Director of the U. S. Geological Survey).

The author explained that the revision

of classification and nomenclature proposed was the result of a resurvey of the formations examined in preparation of the map and folio of the Watkins and Catatonk Quadrangles now in preparation. The section is the standard section of the New York Devonian as originally defined and described by Vanuxem and Hall in 1842–1843. The revision of the classification and nomenclature is based upon a critical study of the composition, sequence and range of the fossil faunas of the Watkins and Catatonk Quadrangles. The thicknesses are estimated as for a generalized section passing through the rocks at Ithaca, N. Y.

The taxonomic classification into series, formations, members and lentils, is in accordance with the rules of the U. S. Geological Survey published in the 24th Annual Report for 1902–1903.

The following chart expresses in brief the classifications and nomenclature presented, the new names having been accepted by the committee on geologic names of the U. S. Geological Survey.

The principal changes affecting nomenclature are the following: viz., the *Nunda shale and flagstone formation* is the stratigraphic equivalent of Hall's Portage or Nunda group, the standard section of which is found in the Genesee Valley. The term Portage was used by Hall for the sandstone member at the top of his Portage or Nunda Group as well as in the name of the other group. Of the two synonyms the first (Portage) is left to be applied in its specific sense to the Portage sandstone member, and Nunda is retained for the formation name.

The lithologically discriminated members of the Nunda characteristic of the Genesee Valley section (*i. e.*, Cachaqua, Gardeau and Portage) are not distinguishable in the Ithaca section. The subdivisions in the latter section are the *Sherburne flagstone*, the *Ithaca shale* and the *Enfield shale members*. The first of these is the equivalent of the Sherburne flagstone of Vanuxem; the second is the typical Ithaca group of Hall and later writers; the third is the

SCHEME OF CLASSIFICATION AND NOMENCLATURE OF THE GEOLOGICAL SECTION PASSING THROUGH ITHACA, N. Y.

	Formations.	Members and Lentils.	Section.	Thickness.
Erie (division or) series of Vanuxem.	*Chemung.* Shale and sandstone formation.	*Fall Creek* conglomerate lentil.		0–10 ft.
		Wellsburg sandstone member.		600–650 ft.
		Cayuta shale member.		600 "
	Nunda. Shale and flagstone formation.	*Enfield* shale member.		550–800 ft.
		Ithaca shale member.		80–300 "
		Sherburne flagstone member.		188–260 "
	Genesee. Shale formation.			125 ft.
	Tully. Limestone formation.			10–30 ft.
	Hamilton. Shale formation.			1,035 ft.
	Marcellus. Shale formation.			125 ft.
	Onondaga. Limestone formation.			125 ft.
	Oriskany. Sandstone formation.			0–4 ft.

'upper part of the Portage' of the classification proposed by Williams in 1884; and the name *Enfield* is applied for the name of the town of Enfield where the member is typically exposed.

The boundary between the Nunda and Chemung formations is established on the basis of change in the generic composition of the faunas occurring at the horizon indicated. The two genera *Dalmanella* and *Douvillina*, and the species *Spirifer disjunctus* are among the more characteristic of the forms marking this change. The *Cayuta shale member* contains the typical Chemung fauna of the Chemung Narrows section; it is paleontologically limited by the Nunda fauna below, and by the fourth of the known zones of *Tropidoleptus* appearing above the Hamilton formation, to which the name *Swartwood Tropidoleptus zone* has been applied by the author for the outcrop southwest of Swartwood at an altitude of about 1,600 feet A. T., containing the fauna.

The name *Cayuta* is applied for the Cayuta Creek, along the sides of which from Cayuta Lake to its discharge into the Susquehanna River typical exposures of the Cayuta member may be seen.

The second member of the Chemung is named *Wellsburg sandstone member* for its outcrop at Wellsburg and above in the hills of Ashland. It is paleontologically discriminated by absence of many common Chemung species and lithologically by the more sandy nature of the rocks; and the thin-bedded slabs characteristic of the higher 'Catskill' rocks are conspicuous in the upper portion.

The Wellsburg member is terminated above by the *Fall Creek conglomerate lentil*, a thin band of conglomerate discovered in place at the top of the Ashland Hills and frequently visible in detached pieces on tops of other hills along the southern townships of the state.

The Fall Creek conglomerate lentil is interpreted to be the equivalent of the conglomerate of that name in Bradford and Tioga Counties, Pennsylvania. In the beds immediately under the conglomerate, and terminating the Wellsburg member, occur in great abundance the shells of *Leptostrophia nervosa* and *Orthothetes chemungensis*. To distinguish this particular horizon the name *Ashland Leptostrophia zone* has been applied to the zone by the author. The author stated that other evidence obtained by comparison of this section with others indicates that the highest stratigraphic extension of the Chemung fauna is not reached at the top of the Wellsburg member, but that to the west and south of this section rocks stratigraphically still higher may bear many of the species of the Chemung formations.

Professor Williams's paper was discussed by Messrs. J. C. Branner, C. A. Hartnagel and D. S. Martin.

Abyssal Igneous Injection as a Causal Condition and as an Effect of Mountain Building: Dr. R. A. DALY, International Boundary Commission, Ottawa, Canada. (Illustrated with a diagram.)

The contraction theory of mountain-building is hypothetically extended to cover the explanation of igneous intrusion, geosynclinal down-warps, the location of mountain-ranges, and the common association of intense mountain-building with the batholithic intrusion of magma. Among the chief postulates are: (1) an earth-crust about twenty-five miles thick, overlying a substratum which, on account of its high temperature, acts as a viscous liquid; (2) the division of the crust into a shell of compression about five miles thick, overlying a shell characterized by cooling tension. The shell of tension extends from

the bottom of the shell of compression down to the substratum.

One effect of cooling tension is to produce cracks in the lower shell; these are only partly closed by the shearing of masses against masses. Because of the solid continuity between the two shells, some of the tension remains unrelieved.

Tension in the lower shell permits of the abyssal (dike) injection of the substratum fluid into the crust. This injection is especially facilitated in earth-zones established by cosmical stresses. The hydrostatic and expansional pressures normal to the walls of such an abyssal dike cause lateral creep and special condensation of matter in the shell of tension. This movement induces a down-warp of the earth's surface overlying the zone of injection. When the down-warp is of large size it is a geosynclinal. Its sedimentary filling is accomplished through the shearing of the shell of compression over the shell of tension. This breaking of the solid continuity between the two shells releases all the accumulated tension. As a result, extraordinarily large bodies of magma are injected from the substratum into the shell of tension beneath the mountain-range. The assimilation of the folded rocks of the range leads to the formation of granitic batholiths.

Dr. Daly's paper was discussed by Messrs. A. C. Lane, J. C. Branner and D. S. Martin.

Brewsters Neck, Connecticut: Dr. F. P. Gulliver, Norwich Town, Conn. (Abstract read by E. O. Hovey.)

Brewsters Neck, Conn., is a deposit of water-laid sand and gravel lying between the preglacial valley of the Thames River, and the preglacial tributary which may be called the Poquetanock River.

The surface of this deposit rises from 70 to 85 feet above the level of the tide water as it exists to-day. It lies from 100 to 150 feet above the level of the bed of the preglacial Thames River.

On the east and west sides of this deposit there is a steep slope. On the north side the surface of the deposit comes directly against the bed rock, which at this point is covered with a thin deposit of till not completely masking the ledges which appear at various places.

On the northwest corner of this deposit there are distinct lobes projecting toward the northwest and between these lobes and the bed-rock slope to the north, which is very steep at this point, there is a deep hollow which is popularly called a 'valley.' The form of this 'valley' shows conclusively that it was not formed by degradation, but by aggradation.

On the southeast corner of this deposit the surface merges gradually into the gentle slope of a low hill, which is bed-rock covered with till. West of this hill is a confused mass of kames and kettle-holes which are beneath sea-level, and since these are now filled with water they are used for floating lumber for the Dawley Lumber Yard at Fort Point.

Perhaps the most interesting point along the margins of these deposits lies just above these two salt-water ponds, which fill these kettle-holes. The surface of this deposit, which the author would name a *delta-terrace,* slopes from the northeast, where the highest point is 85 feet above the level of the sea, toward the southwest, where the surface is about 70 feet. The deposit is undoubtedly a glacial delta. Its distinct lobes and structure are typically that found in deposits laid by a stream emptying into standing water. It is very similar to the structure reported by the author of this paper as occurring at the Navy Yard, above New London.[1]

[1] 'Thames River Terraces in Connecticut,' *Bull. Geol. Soc. Am.,* Vol. 10, 1898, pp. 492–494.

A new point in regard to the theory of the formation of water-laid glacial deposits is believed to be as follows: When the margin of the ice-sheet was retreating by the annual summer melting faster than ice was supplied, the ice remained longest in the valleys. Masses of ice would thus be left in the deeper valleys which would rise many feet above the surrounding hills. Between these ice masses standing water would occur either as arms of the ocean or as ice-dammed glacial lakes between these ice-blocks. In these bodies of standing water the streams from the melting ice would deposit their sand and gravel, and at the point where the streams issued from the ice there would be eskers and ice-contact slopes. These latter deposits would contain the larger materials, and the fine sand would be found farthest from the point where the streams issued from the ice. Where the supply of rock waste was sufficient to fill up the space lying between these ice-blocks, a flat topped delta-terrace would be formed; where the supply was not sufficient to fill up the water body, lobes would occur. These lobes would project into the water body, and from the axis of the arched layers as shown in sections, the direction of flow taken by a given glacial distributary may be discovered.

In this deposit, therefore, the lobes at the northwest corner show that the sand of which they are built was coming from the southeast. The arched layers shown in the sections at the southwest corner of this deposit indicate that the material there found came from the northeast. Other sections of this delta-terrace at Brewsters Neck, as revealed by the excavations made for the State Hospital, show that in the center of the deposit there were alternating currents coming from both the east and the west.

The author believes from the facts shown by these recent cuttings and from the facts which have long been recognized from the surface form, that this deposit was built by streams issuing from two blocks of ice, one of these blocks occupying the valley of the glacial Thames, the other occupying the valley of the tributary of the Thames River which flowed through the Poquetanock Valley. The slope of the surface of this delta-terrace indicates that the last source of the material was from the Poquetanock ice-block on the east.

Notes on the Geology of the Guaynopita, Chihuahua, Mexico, Mining District: Dr. E. O. HOVEY, American Museum of Natural History, New York. (Illustrated with lantern slides.)

The Guaynopita district lies in the heart of the Western Sierra Madre in northern Mexico. It shows a series of Cretaceous limestone and schist and gneiss overlain by volcanic rocks (andesites) and invaded by granite intrusions. Later volcanics (basalt, andesites and rhyolites) have supervened, basins have been filled with sandstones and conglomerates, and the whole region has been deeply dissected by the Aros and its tributaries.

Dr. Hovey's paper was discussed by Messrs. A. C. Lane and C. R. Dryer.

The Relations of the Drainage of the Santa Clara Valley, California, to that of the Pajaro River: Professor J. C. BRANNER, Stanford University.

Several years ago Dr. Joseph Le Conte advanced the theory that the drainage of the Sacramento Valley of California formerly entered the Pacific Ocean through the Bay of Monterey instead of through the Golden Gate as it does at present. This theory was based chiefly upon the absence of a submerged channel outside of the Golden Gate and the presence of such a channel in the Bay of Monterey. Later

the study of the fish faunas of the streams of California by Professor C. H. Gilbert and his colleagues showed that there had formerly been some connection between the Sacramento, the Coyote, the Pajaro, the Salinas and various other streams that are now separate. These faunal relations seemed to give strong support to the theory of the newness of the Golden Gate channel.

Later study shows that the mingling of these faunas may be explained otherwise. During the glacial epoch the region in question stood from 2,000 to 3,000 feet higher than it does at present. The Bay of San Francisco did not then exist, but the Sacramento and Coyote drainage flowed through it. Coyote Creek rising on the western slope of the Mt. Hamilton range flowed into the Santa Clara Valley at the top of the watershed between the present Coyote Creek and the Pajaro drainage. An alluvial cone was built up on the flat valley-floor where the Coyote emerged on the plain of the Santa Clara Valley. As this cone grew the Coyote swung from side to side, emptying its waters now into the Bay of San Francisco and now into the Bay of Monterey. Fishes ascending from the Sacramento into the Mt. Hamilton range, could later descend into the Pajaro drainage. The elevation of the coast at that period carried the mouth of the Pajaro farther toward the west, so that fishes could pass from the Pajaro into the San Lorenzo and into the Salinas without entering the sea.

The depression following the glacial epoch submerged the mouth of the Sacramento forming the Bay of San Francisco, and separated the San Lorenzo from the Pajaro and the Salinas Rivers. The decreased precipitation diminished the volume of Coyote Creek and it ceased to build the cone near Madrone Station, and since then it has flowed toward the Bay of San Francisco and has been cutting into its alluvial cone.

Professor Branner's paper was discussed by Messrs. T. C. Hopkins, C. R. Dryer, A. C. Lane, George H. Chadwick and E. O. Hovey.

The Geology of Coon Butte, Arizona: Messrs. D. M. Barringer and B. C. Tilghman, Coon Butte, Arizona. (Illustrated with diagrams and specimens and presented by J. C. Branner.)

Coon Butte, Ariz., is a low crater-like hill six miles south of Sunshine Station on the Santa Fe Railway, and about twenty-five miles southeast of Flagstaff. The general geology of the surrounding country is very simple, consisting of horizontal beds of red sandstone, silicious limestone, white sandstone and light brown sandstone, each bed being clearly distinguished from the others. In this plain is a pit about 600 feet deep and three quarters of a mile across. The edges of the beds exposed in the pit are all turned sharply upwards. The slopes of the ridge around the pit are covered with blocks and fragments of rocks like those exposed within the pit. Several years ago Mr. G. K. Gilbert made a study of Coon Butte. He appears to have supposed at first that it was formed by a meteor striking the earth. His final conclusion was, however, that it was caused by a steam explosion.

Some years ago a mining company acting upon the theory that the Coon Butte pit was made by a meteor, began a systematic search for the mass in the bottom of the crater-like depression. Their explorations show that the region has yielded several tons of meteoric iron, and that a large number of pieces of this iron have been found interbedded with the débris thrown from the pit. The distribution of the irons on the surrounding plain also lends strong support to the idea that the depression was

made by a meteor striking the earth. The bottom of the pit is filled with fossiliferous fresh-water deposits to a depth of about eighty feet.

Messrs. Barringer's and Tilghman's paper was discussed by Messrs. J. C. Branner, A. C. Lane and E. O. Hovey.

Recess was taken from 12:45 to 3:45 on account of lunch and the dedication of Rockefeller Hall.

Saturday, June 30. Excursion to Union Springs under the leadership of Professor G. D. Harris, of Cornell University. This was participated in by thirteen members of the section. The stratigraphy, paleontology and physiography of the region were studied.

Monday, July 2. An excursion by boat to various points of interest at the southern end of Cayuga Lake under the leadership of Professor G. D. Harris, which was participated in by eighteen members of the section and association. The succession of Devonian beds along the lake shores was noted; peculiarities of shore-line and delta formations observed and the erosion of the strata as exemplified in the Taughannock Falls studied. The Glenwood intrusive dike was visited.

Tuesday, July 3. An excursion to Enfield Glen under the leadership of R. H. Whitbeck of the State Normal School, Trenton, N. J., which was participated in by fifteen members of the section and association. The glacial phenomena of Cayuga Valley, the geology of the lateral valleys, the jointed structure of the rocks and the Enfield Falls, were the natural features of particular interest.

After lunch at the Enfield Glen Professor C. R. Dryer, of the State Normal School, Terra Haute, Ind., gave, by invitation, a carefully prepared paper upon 'The Geologic Features of the Finger Lake Region, New York.'

Western New York may be divided into four physiographic regions: (1) the Iroquois plain, (2) the drumlin belt, (3) the Finger Lake belt, (4) the Allegheny Plateau. The Finger Lake belt lies on the northward slope of the plateau and is trenched by numerous north-south valleys, many of which contain lakes. The valleys are long, narrow, straight or but slightly curved, flat-bottomed, from 1,500 to 2,500 deep and partly filled with sediment. The rock floor of the Cayuga Valley is 55 feet below the sea level and that of the Seneca Valley at least 556 feet. The slopes of the valley walls are smooth, symmetrical, devoid of spurs, and below the 900-foot level are sharply oversteepened. The wide, open, mature valleys of the tributary streams are, with a few exceptions, 'hung up' at the edge of the steepened slope and the streams drop by rapids and falls through narrow rock gorges, to the lake level. Enfield Creek has in its upper course a fall of about 30 feet to the mile, then it makes a nearly vertical fall of 210 feet and descends 450 feet in one mile and a half to the level of Cayuga Inlet. If the slope of the upper part be extended out into the air it would pass 500 feet above the present mouth of the stream. Within sight on the southern horizon is a notch or gap 600 feet deep and one fourth of a mile wide which leads from the head of this stream through to the Chemung drainage without any definite divide.

The physiographic problems of this region are many; but the features just enumerated can have but one meaning. Main valleys oversteepened, overdeepened and too straight and smooth to be the work of streams alone, tributary valleys hanging hundreds of feet above their immediate baselevel, a main divide notched by through-going gaps—these are as characteristic of glaciated and ice-shaped regions as

the features of Alaska, the Alps, Norway or New Zealand. In face of the fact that such peculiar, strongly marked and abnormal features always occur concordantly in regions known to have been extensively glaciated, any other hypothesis than that they are due directly or immediately to ice action of some kind seems gratuitous and impertinent. To set up such trifles as a patch of residual soil, an island in the lake, a shore cliff, etc., as competent to negative the evidences of glacial erosion is to swallow gnats and strain out a camel. A gnat may be as difficult to explain as a camel, but it is of relatively little consequence whether he is explained or not. As a means of progress across the desert of hypothesis to the oasis of conclusion, the camel may be relied on and the gnats that annoy him disregarded. The various sessions and excursions of the section were attended by thirty different members of the association.

The section finally adjourned at about 4 P.M., Tuesday, July 3.

Edmund Otis Hovey,
Secretary.

SCIENTIFIC BOOKS.

Zur Erkenntnis der Kolloide. By Richard Zsigmondy. Jena, Gustav Fischer. 1905. Pp. vi + 185. 4 Marks.

The names of Siedentopf and Zsigmondy have become familiar to the whole scientific world through the brilliant researches which resulted in the development of the 'ultra apparatus'; their device for observing particles so small as almost to reach the hypothetical molecular dimensions.[1]

In this monograph, 'Zur Erkenntnis der Kolloide,' Zsigmondy has given a careful account of his own and Siedentopf's methods and results, a brief historical review of many other noteworthy researches and a short discussion of the bearing of the 'ultra' methods upon certain theoretical issues.

He states that the main purpose of his researches on colloids has been to determine whether or not the polarization and dispersion of light in Tyndall's experiment is an essential characteristic of all hydrosols or colloidal solutions. He has answered this question in the affirmative in numerous cases, having demonstrated that the dispersion of light is due to the presence of the same particles to which are to be ascribed the other remarkable properties of the liquids containing them.

It may not be amiss to state briefly the principle of Tyndall's experiment and of the 'ultra apparatus,' and to recount some of the interesting facts described in the book. A beam of light is sent through the space under observation, and the observer looks at the space in a direction perpendicular to the course of the beam. If dust, or other fine particles, are present in that space, they polarize, disperse and reflect the light, and the beam is seen. If no such particles are present, the beam is not seen, and the space is said to be optically empty.

With all the resources of the famous firm of Carl Zeiss at Jena at their command, Siedentopf and Zsigmondy constructed their apparatus to send a beam of great intensity, a minute image of the sun, into the medium under investigation, and observed through the best of microscopes. Under these conditions, the particles of metallic gold in a colloidal gold solution appear as brilliant sources of light, but their shape can not be determined.

Siedentopf and Zsigmondy counted the number of these bright spots in a space of known dimensions, they knew the concentration of the gold solution (the weight of gold per unit volume); they assumed that all the gold present was visible, that it had the same specific gravity in this finely divided state as when massive, and that the particles were cubes. From these values they easily calculated the linear dimensions of the individual particles. It will be seen at once that these dimensions can not be regarded as ascertained, because of the assumptions, but

[1] The original articles appeared in the *Zeitschrift für Elektrochemie*, Vol. VIII., pp. 684–687 (1902) and in Drude's Annalen, Vol. X., pp. 1–39 (1903).

they possess a high degree of probability. The smallest particles which they were able to count appeared to have a linear dimension of 6 $\mu\mu$ (millimicrons, or thousandths of a micron).

The extraordinary character of this achievement, actually counting such small particles, is best realized through comparisons. The smallest object visible with the best microscope has a linear dimension of about $\frac{1}{4}$ μ or 250 $\mu\mu$. The hypothetical diameter of a molecule of soluble starch has been estimated by Lobry de Bruyn at about 5 $\mu\mu$ and of a hydrogen molecule by O. E. Meyer at about 0.1 $\mu\mu$. On the other hand a human blood corpuscle has a diameter of about 7.5 μ or 7,500 $\mu\mu$. The diagrams in the book make these comparisons yet more striking.

Some solutions contain smaller particles, as is evident from the fact that the course of the light can be traced, although the individual particles can not be counted. Many solutions appear optically empty even under this searching test. We may wish to define true solutions as those which are optically empty under the 'ultra apparatus,' and colloidal solutions as those which are not optically empty under the same conditions. But there are countless intermediate steps, and these observations raise the natural question whether, after all, there is any difference between true and colloidal solutions, except in degree. That we fail to find lack of homogeneity in what we call true solutions may be due merely to the limitations of our apparatus. One naturally wonders if we may not some day, by these methods, be able to answer, in terms of fact instead of theory, those puzzling questions; in what form is a substance when in solution, and why are some things soluble and other things practically insoluble in given solvents?

Siedentopf and Zsigmondy consider it desirable to coin some new words to cover the new region which they have brought within the range of direct observation. They suggest that all particles smaller than $\frac{1}{4}$ μ shall be called ultra-microscopic particles or, for the sake of brevity, ultra-microns. These are to be subdivided into two groups, (1) those which may be counted in the 'ultra apparatus,' to be called sub-microscopic particles, submicrons, or hypo-microns, (2) those which may not be so counted, to be called amicroscopic particles or amicrons.

The sub-microns, in water solutions, possess lasting and extremely rapid oscillatory and translatory motions which differ from the typical Brownian motion in several particulars. The description of this motion, illustrated with figures, is interesting.

As is well known, we have blue solutions of colloidal gold as well as red. Zsigmondy concludes, in harmony with the majority of other authorities, that there is no observable connection between the color and the size of the particles.

Ruby glass is unquestionably to be considered as a supercooled colloidal solution. It is possible to obtain colorless 'ruby glass' by cooling, with sufficient rapidity, the solution of metallic gold in molten glass. This colorless glass, upon being heated to a temperature at which it softens, turns red. Zsigmondy devotes a good deal of space to a theoretical discussion of how the amicrons accumulate at centers to form submicrons and small crystals.

A clue is obtained to the size of the pores of filters by determining the size of gold particles which have passed through them. For instance, sub-microns of gold with linear dimensions of 30 $\mu\mu$ passed through Maasen, Pukal and Chamberland filters, which must then have some pores of that size.

The book closes with the well-justified statement that the 'ultra' methods form an important addition to, and extension of, other physical and chemical methods for the study of conditions which have hitherto escaped direct observation. It can not be considered an exhaustive review of the whole subject of colloids. Lottermoser's 'Ueber anorganische Colloïde' in the sixth volume of Ahren's Sammlung chemischer und chemisch-technischer Vorträge is a more complete summary of the literature. But Zsigmondy has given us an eminently satisfactory, and most welcome treatise on the 'ultra' methods, and the

deeply interesting results obtained by them. Moreover, it is suggestive of many possibilities of future discovery.

S. Lawrence Bigelow.

SCIENTIFIC JOURNALS AND ARTICLES.

The Journal of Experimental Zoology, Vol. III., No. 2 (July, 1906), contains the following articles: 'Observations and Experiments Concerning the Elementary Phenomena of Embryonic Development in *Chætopterus,*' by Frank R. Lillie. This is a study of the origin and behavior of formative stuffs in the early development of an annelid by direct observation, and by various experiments, *e. g.*, analysis by centrifugal force, by staining intra vitam, and by suppressing the process of cleavage without prejudice to other embryonic processes. The standpoint is that a complete account of embryonic development would trace everything back to the chromosome complex of the species. 'Regeneration of Grafted Pieces of Planarians,' by Lilian V. Morgan. A complete head may regenerate from a posterior cut surface of planarians if a very short piece is grafted in a reverse direction on a larger piece. 'Experiments on the Behavior of Tubicolous Annelids,' by Charles W. Hargitt. 'Inheritance of Dichromatism in *Lina* and *Gastroidea,*' by Isabel McCracken, Stanford University. In this paper the author records the results of an attempt to determine the behavior in heredity of the alternate characters in dichromatic species. Two dichromatic beetles, *Lina lapponica* and *Gastroidea dissimilis,* were bred under controlled conditions through a series of generations, four in the former, seven in the latter. The investigator finds an accumulative dominance of one color over the other from generation to generation, or a prepotency of the dominating color that apparently eventually eliminates the recessive color from the dominant line. The recessive color behaves like a typical Mendelian recessive.

DISCUSSION AND CORRESPONDENCE.

WHEN DID FRANKLIN INVENT THE LIGHTNING-ROD?

The bi-centenary of Benjamin Franklin's birth has served to recall attention to the varied achievements of this remarkable man, but it would hardly be expected that new facts could be learned regarding the invention of the lightning-rod, upon which his popular fame as a natural philosopher chiefly rests.

Franklin's classic experiment with the electrical kite, by which he demonstrated the identity of lightning and artificial electricity, was performed at Philadelphia during the summer of 1752. The date June, which is frequently quoted, seems to have been authorized by Priestley in his *History of Electricity.* On the contrary, his French contemporary, De Romas, who claimed the idea of the electrical kite, maintained that Franklin did not fly his kite in June, nor until after he had heard of the success of the French experimenters, Dalibard and Delor, who, in May, 1752, collected the electricity during a thunderstorm by metal rods, according to a method which he himself had suggested. Authorities differ as to whether Franklin knew of this when he obtained the same results with his kite, Park Benjamin, on page 589 of his 'Intellectual Rise in Electricity,' asserting that Franklin desired to confirm the French experiments. If this be true the kite experiment could hardly have been executed at Philadelphia so soon as the following month, that is in June, but, at all events, no mention of it occurs anywhere until a letter describing it, written there in October to Peter Collinson at London, was read before the Royal Society on December 21, 1752. This communication, which appeared in the *Gentleman's Magazine* for December, 1752, and in the *Philosophical Transactions* for the same year, was reprinted with Franklin's 'Experiments and Observations in Electricity,' of which the second part of the first edition was published at London in 1753. While it seems to have passed unnoticed that the letter describing the electrical kite in the *Philosophical Transactions* is dated October 1 and the same letter in the collected papers bears the date October 19, a date subsequently adopted by his biographers, it was reserved for a German bibliographer, Professor Hellmann, to point out, in publishing a facsimile of this

letter from the *Philosophical Transactions*, in No. 11 of *Neudrucke von Schriften und Karten über Meteorologie und Erdmagnetismus* (Berlin, 1898), that an important omission had been made in the slightly different version printed in the collected papers. The present writer finds that this error has been perpetuated by such careful biographers of Franklin as Sparks, Bigelow and Smyth, who, having reprinted the letter from the collected papers, also omit this concluding paragraph:

I was pleased to hear of the success of my experiments in France, and that they there begin to erect points upon their buildings. We had before placed them upon our academy and state-house spires.

Professor Hellmann supposes that Franklin, by the last statement, wished to claim priority in the use of pointed rods as lightning conductors.

Already in his 'Opinions and Conjectures Concerning the Properties and Effects of the Electrical Matters, arising from Experiments and Observations made at Philadelphia, 1749,' contained in a letter to Peter Collinson, dated July 29, 1750, Franklin had proposed to test the electricity of thunder-clouds and had suggested the possibility of the lightning-rod. In order to continue his experiments, Franklin, in September, 1752, erected on his house an insulated iron rod, connected at its lower end with a pair of bells which, by ringing, would show that the rod was electrified. On April 17, 1753, he charged one Leyden jar from this rod and another jar with positive electricity from a frictional machine, concluding from this and subsequent experiments

That the clouds of a thunder-gust are most commonly in a negative state of electricity, but sometimes in a positive state.

Further on in the same letter in which the preceding experiments were described, written at Philadelphia in September, 1753, Franklin says:

Metalline rods, therefore, of sufficient thickness, and extending from the highest part of an edifice to the ground, being of the best material and complete conductors, will, I think, secure the building from damage, either by restoring the equilibrium so fast as to prevent a stroke, or by conducting it in the substance of the rod so far as the rod goes so that there shall be no explosion but what is above its point, between that and the clouds.

This is generally believed to be the first definite announcement of the lightning-rod, although Poggendorff, in his 'Geschichte der Physik,' page 867, casts doubt on Franklin's sole claim to its invention by saying that J. H. Winkler, of Leipzig, in his 'Programma de avertendi fulminis artificio,' Lipsia, 1753, recommended the use of lightning conductors and gave directions for their erection, in consequence of which, probably, they were first introduced into Germany in 1754.

I shall show, however, by what follows that Franklin had prepared detailed instructions for the installation of lightning-rods nearly a year before he wrote the letter that has just been quoted. In *Poor Richard's (Improved) Almanac* for 1753, edited by Richard Saunders (Benjamin Franklin's pseudonym) and published in Philadelphia by B. Franklin and D. Hall, the material for which must have been ready not later than October, 1752, occurs the following remarkable article which seems to have entirely escaped the attention of Franklin's biographers and of scientific students generally.

How to secure Houses, &c. from Lightning. It has pleased God in His Goodness to Mankind, at length to discover to them the Means of securing their Habitations and other Buildings from Mischief by Thunder and Lightning. The Method is this: Provide a small Iron Rod (it may be made of the Rod-iron used by the Nailers) but of such a Length, that one End being three or four Feet in the moist Ground the other may be six or eight Feet above the highest Part of the Building. To the upper End of the Rod fasten about a Foot of Brass Wire, the size of a common Knitting-needle, sharpened to a fine Point; the Rod may be secured to the House by a few small Staples. If the House or Barn be long, there may be a Rod and Point at each End, and a middling Wire along the Ridge from one to the other. A House thus furnished will not be damaged by Lightning, it being attracted by the Points and passing thro the Metal into the Ground without hurting any Thing. Vessels also,

having a sharp pointed Rod fix'd on the Top of their Masts, with a Wire from the Foot of the Rod reaching down, round one of the Shrouds, to the Water, will not be hurt by Lightning.

From the foregoing it is evident that Franklin drew up definite instructions for erecting lightning-rods before the close of the year 1752 and from the contemporaneous letter, describing the electrical kite, it would appear that at this time some edifices in Philadelphia were thus equipped, but whether for their protection or for experiment is uncertain.

A. Lawrence Rotch.
Blue Hill Meteorological Observatory,
August 13, 1906.

DRIED COTTON CULTURES ONCE MORE.

It would appear from a recent communication in Science that the original dictum of Messrs. Harding and Prucha that the nodule-forming bacteria will not survive when dried upon cotton is now to be modified to apply only to those cultures dried (?) in a Petri dish enclosed in a paper bag and stored in a drawer or under conditions considered to be similar to this. Although an attempt is made to show that such a method closely resembles that used in the original preparation of cotton cultures, the actual difference in drying time will be so evident to any one who cares to try the experiment that it need not be discussed here. Indeed, it would hardly seem necessary to demonstrate the fact that of two pieces of cotton containing the same amount of moisture, one enclosed in a Petri dish, and the other exposed to the action of an abundance of warm air, the one in the air will dry much sooner than the protected piece. One would as readily expect clothes to dry as rapidly enclosed in a box, wrapped in paper and put away in a drawer as when hung upon a line. In fact it was this very question of rapidity of drying which caused the various 'germ-proof' methods of preparing the cotton to be abandoned. It was found that even in a specially constructed box of large size (approximately 8 x 2 x 2 feet) through which air was forced over a warm coil, that the length of time required for drying was entirely too long, and for practical purposes, considering the way in which the cultures were to be used, it was better policy to turn out a culture which might be more or less contaminated than to destroy the nodule-forming bacteria in an attempt to produce a pure culture. Experiments showed that all attempts, with the facilities available, to produce absolutely pure cultures upon cotton involved an unwarranted sacrifice of efficiency.

It has been supposed that the result obtained by the users of the dried cotton cultures would of itself be a sufficient refutation of the statement that dried cotton cultures were valueless. This seems to be the case since an attempt is now made to explain the reports received by users of the cultures upon 'psychological' grounds.

If the bacteria will not live upon cotton it is of course necessary to account in some way for the thousands of satisfactory reports received from this country and elsewhere, and the psychological explanation would seem to be as good as any. It should be remembered, however, that many of the favorable reports were furnished by those fully as competent to judge of the results obtained as a member of an experiment-station staff—that a considerable number of these experiments were checked by uninoculated plots and that photographs showing the difference between these treated and untreated plots are available in all cases where it was possible to obtain them. Furthermore, the reports covered tests with sterilized soil and sand as well as field experiments.

Since the publication of a few of the favorable reports first received, there have been many others which emphasize even more strongly the benefit derived from cultures when used under the conditions for which they are designed. During the past season, while the statement that the cotton cultures were valueless was very widely published, producing, one would suppose, anything but a favorable psychological effect, most satisfactory reports continued to be received.

Although it is manifestly impossible in this statement to refer directly to any of these reports, it may not be out of place to cite

briefly the results obtained with dried cotton cultures in competition with pure cultures on agar obtained from Hiltner of Munich, by the Board of Agriculture of Great Britain. This test was conducted by thirteen different agricultural colleges and experiment stations during the year 1905. The general plan of the experiment was as follows:

1. Laboratory experiments with sterilized soil and sand.

2. Pot cultures in unsterilized soils from various sources.

3. Experiments in accord with actual agricultural or horticultural practise.

The results of these experiments are published in detail, and to any one examining them it can not but be evident that the contention that the nodule-forming bacteria properly dried upon cotton will not live is entirely erroneous. In spite of the fact that the cotton cultures were in some instances a year old and had undoubtedly been subjected to various unfavorable conditions, there was no advantage whatever in the Hiltner fresh pure culture over the dried cotton culture. The conclusion reached by the board speaks for itself:

The experiments in which positive results were obtained show that in many cases both Moore's and Hiltner's cultures were alive, and capable of infecting the plants for which they were designed and producing an increased yield.

Concerning the statement that no United States experiment station has been able to obtain 'good results' from dried cotton cultures it would seem that in view of the results obtained either by experiment stations, or under government supervision, in Great Britain, Australia, New Zealand, South Africa, China and other foreign countries—to say nothing of the unquestionable ability of those reporting success in this country—that the 'psychological' explanation should be applied to the experiment station reports rather than to those obtained from successful users of dried cotton cultures. Furthermore, an analysis of the reports from those experiment stations which I have had an opportunity to examine does not reveal the worthlessness of the cultures which it seems so desirable to demonstrate, and finally it is not an impossibility that under the circumstances the experiment station may not be the final court of appeal in such a matter, after all. One would hardly expect 'any well-marked result' from a culture which, after being prepared, was carefully kept upon ice until ready for use—a method actually employed at one of the experiment stations.

The question of the vitality of nodule-forming bacteria in dried cotton cultures is one that can not be settled by an attempt to explain away the actual results which can be and have been obtained from their use. The incompatibility of these results with the idea that the bacteria are killed by drying is admitted, but it is not the results which will have to fall. I shall be glad to send dried cotton cultures to any one who may wish to arrive at an independent conclusion regarding the ability of the nodule-forming bacteria to withstand drying. These cultures may be retained any length of time up to twelve months before testing, so there will be no question about the period during which vitality is retained. The inoculated cotton will have been dried in a room through which a current of air has been forced, not in a Petri dish.

GEORGE T. MOORE.
MARINE BIOLOGICAL LABORATORY,
WOODS HOLE, MASS.,
July 28, 1906.

SPECIAL ARTICLES.

GEOLOGY OF SOUTH BRAZIL.

[THE following note from Dr. I. C. White, chief of the Brazilian Coal Commission, should have appeared in connection with the communication of Professor Osborn on 'Vertebrate Paleontology,' published in the issue of SCIENCE of August 3, but was received too late for insertion in that issue.]

Mesosaurus Brasiliensis occurs in the Iraty black shale, state of Paraná, a formation whose base lies about 100 meters above the Brazilian Coal Measures which hold the typical Glossopteris flora of the Ecca series of South Africa, and the Karharbari series of India. It is

also the same horizon at which Dr. Derby finds *Stereosternum tumidum* in the state of São Paulo, and its geological horizon is approximately 300 meters below the *red beds* that hold reptilian remains (*Scaphonyx fischeri*) which Dr. A. Smith Woodward of the British Museum regards as closely related to *Euskelesaurus* of the Stormberg, or Triassic beds of South Africa. In fact, it appears that we have in Brazil a series of rocks which correlate closely with the Karroo system of South Africa, and that glacial conditions also existed in Brazil immediately preceding the deposition of the Coal Measures there, during the epoch of the Orleans Conglomerate which corresponds to the 'Dwyka,' and 'Talchir' conglomerates of Africa and India. The following scheme of classification, which I have adopted for the beds near Minas, Santa Catharina, will give a general idea of the stratigraphic succession in that state.

Santa Catharina System:	São Bento Series:	Serra Geral eruptives.
		Botucatú sandstones, great cliffs of red, gray and cream-colored sandstones.
		Rio do Rasto red beds, with fossil reptiles (*Scaphonyx*) and fossil trees.
	Passa Dois Series:	Rocinha limestone.
		Estrada Nova gray and variegated shales with cherty concretions, and sandy beds.
		Iraty black shale, *Mesosaurus* and *Stereosternum*.
	Tubarão Series:	Palermo shales.
		Rio Bonito shales and sandstones, with Coal Measures and *Glossopteris* (*Gangamopteris*) flora.
		Orleans conglomerate.
		Yellow sandstones and shales to granite floor.

The Tubarão and Passa Dois series would correlate with the Ecca series, or Lower Karroo, of South Africa, while the São Bento beds would represent the Stormberg, or Upper Karroo series. The fossils of the Beaufort or Middle Karroo series have not yet been discovered in South Brazil.

Mr. David White, of the United States Geological Survey, to whom was entrusted the study and description of the fossil plants collected from the rocks of the Santa Catharina system by the Brazilian Coal Commission, finds the typical *Glossopteris* flora in the collections from the Tubarão and Passa Dois series, and assigns both to the Permian. This conclusion is shared by the writer except that he thinks it possible the Coal Measures or Rio Bonito beds of the series given above may be of Permo-Carboniferous or Artinskian age, while all above them to the base of the São Bento series would be true Permian, and this would of course include the *Mesosaurus* zone. The very interesting and elaborate memoir of Mr. David White, illustrated with ten plates of fossil plants, will be published, along with Dr. J. H. McGregor's valuable memoir on *Mesosaurus brasiliensis*, in my final report to the Brazilian government on its coal resources. The name 'Gangamopteris flora' is preferred to that of 'Glossopteris flora' by Mr. David White for reasons that he fully explains in connection with his description of this interesting flora.

The following quotation from Mr. David White's manuscript, giving the list of species, will prove of much interest:

> That the flora of the Brazilian coal measures is composed of mingled older Gondwana and Northern Permian species has long been known. The presence of Gondwana elements in Brazil was recognized by Hettner; and Zeiller has contributed a very valuable discussion of the intermingling of Northern and Southern types, and of the consequent deductions as to the boundaries of the two great floral provinces in Permo-Carboniferous time. In the following pages it will be shown that we have in southern Brazil a typical Lower Gondwana flora, embracing the principal types characteristic of that flora in India, Australia and South Africa, to which is added a smaller part, chiefly composed of Lepidophytes, derived more or less directly from the Northern Permo-Carboniferous flora. Also

it will be seen that the Gondwana forms were present in the lowest sediments of the series, while the Northern elements, appearing a little later, seem to become more abundant and varied in the higher beds.

The total flora, so far as yet discovered, is in the following

LIST OF THE FOSSIL PLANTS FROM THE COAL-FIELDS OF BRAZIL.

Reinschia australis Bert. & Ben., var. *brasiliensis* n. var.
Rosellinites Gangamopteridis n. sp.
Hysterites brasiliensis n. sp.
Equisetites calamitinoides n. sp.
Schizoneura? sp.
Phyllotheca Griesbachi Zeill.
Phyllotheca Muelleriana n. sp.
Phyllotheca (*?*) sp.
Lycopodiopsis Derbyi Re.
Lepidodendron Pedroanum (Carr.) Zeill.
Lepidophloios laricinus Sternb.
Sigillaria Brardii Brongn.
Sigillaria australis n. sp.
Sigillaria sp.
Sigillaria (*?*) *muralis* n. sp.
Sphenopteris hastata McCoy?
Sphenopteris sp.
Psaronius brasiliensis Brongn. (Not represented in the collection.)
Neuropteridium Plantianum (Carr.) D. W. (Not represented in the collection.)
Glossopteris Browniana Brongn.
Glossopteris indica (Brongn.) Schimp.
Glossopteris ampla Dana.
Glossopteris occidentalis n. sp.
Glossopteris sp.
Vertebraria sp.
Gangamopteris obovata (Carr.) D. W.
Ottokaria ovalis n. sp.
Arberia minasica n. g., n. sp.
Derbyella aurita n. g., n. sp.
Nœggerathiopsis Hislopi (Bunb.) Feist.
Cardiocarpon Seixasi n. sp.
Cardiocarpon Moreiranum n. sp.
Cardiocarpon Oliveiranum n. sp.
Cardiocarpon Barcellosum n. sp.
Voltzia? sp.
Dadoxylon Pedroi Zeill.
Dadoxylon nummularium n. sp.
Dadoxylon meridionale n. sp.
Carpolithus? sp.
Hastimima Whitei n. g., n. sp.

Although the number of forms, forty in all, is not large, it will at once be noted that it embraces representatives of the four dominant and characteristic groups of the older Gondwana flora; namely, *Phyllotheca, Glossopteris, Gangamopteris* and *Nœggerathiopsis,* all of which are present in identical oriental species.

I. C. WHITE.

MORGANTOWN, W. VA.,
August 1, 1906.

THE FOREST DISTRICTS OF UGANDA.

A journey of inspection through the most important forest areas of the Uganda protectorate has lately been made by Mr. M. T. Dawe, officer in charge of the forestry and scientific department, who gives the results of his observations in a parliamentary paper, according to the abstract in *The Geographical Journal.* The largest forests (as shown in a sketch-map accompanying the report) lie along two strips of country, one running near the west and northwest shores of the Victoria Nyanza, the other following a more or less parallel line through the extreme west of the protectorate. Mr. Dawe describes each forest in turn, and though his point of view is mainly economic, many details of scientific interest can be gleaned from the report. A discovery was that of the Lagos silk rubber tree (*Funtumia elastica*) as an indigenous element, while several new species of *Landolphia,* some of economic importance, were also brought to light. Of the first forests visited, that of Bujeju in Buddu occupies a low-lying tract near the lake, some parts of which seem once to have formed part of the latter. The forest seems comparatively new, and to be gradually encroaching on the interior plains. It contains no good rubber-vines, though they are found in adjoining areas. One of the trees found in this district, though not of large size, is a variety of the valuable *Podocarpus milanjianus,* which occurs practically on the lake-shore. Of the western forests, that of western Ankole occupies a large area east of Lake Albert Edward. It is largely composed of three trees—*Carapa grandiflora, Symphonia globulifera* and *Parinarium excelsum.* Mr. Dawe remarks that almost every forest in Uganda possesses its own special character,

due to the predominance of one or two particular trees. In Toro, the Kibale forest covers a considerable area. The most common tree is one nearly allied to *Maba abyssinica,* with ebony-like markings in the center. An interesting tree with resinous bark and a timber much resembling sandal-wood proved to be unknown, and has been placed in a new genus (*Dawea*), as well as in a new tribe of *Bixineæ*. To the south this forest touches the northern end of Lake Ruisamba. An account is given of the types of vegetation on the Ruwenzori range, agreeing generally with those of previous travelers, though more detailed as regards species met with. Here, too, the *Podocarpus milanjianus* occurs. The tree-*Senecio* growing near the lower glaciers proved to be a new species, which has been named *adnivalis*. From the point reached near the base of the glacier no snow was visible (in July), and Mr. Dawe concludes that there are much colder seasons. The country comprising the Semliki plains, with the Ruwenzori and Mboga ranges on either side, is described as one of the finest districts for cultivation in the protectorate, the hot Semliki valley being especially adapted for tropical products, such as Para rubber. The climate of the Mboga hills seems more bracing than that of Ruwenzori. In the Semliki forest (which is largely composed of *Cynometra*), the oil palm of West Africa is found, and is said to be abundant in certain localities. The Bwamba of this region are very destructive to forest land, as they are constantly making new clearings. In the Bugoma forest of Unyoro the *Cynometra Alexandri* is again abundant, and where this is the case there is little rubber. Further north, the Budongo forest is said to cover an area of 350 square miles. It is well watered. The growth of trees is very dense, and it is the most valuable timber forest seen in the protectorate, the most important trees belonging to the mahogany order. Unyoro is regarded by Mr. Dawe as offering great possibilities for agriculture, and as admirably adapted for cotton so far as the soil is concerned.

APPOINTMENTS AND REMOVALS AT STANFORD UNIVERSITY.[1]

It is believed by the president and members of the board of trustees that the vesting of the power of removal of members of the university solely in the hands of the president in accordance with the original deed of grant is an arrangement not desirable as a permanency in university management. Furthermore, there seems no good reason why appointments, promotions and removals should not stand on the same footing. In view of this, the following resolution was adopted by the board of trustees of the university, on March 30, 1906, with the full approval of the president. This resolution, although properly belonging to the report for 1906–07, is on account of its importance incorporated here, the delay in printing the present report making this possible.

Whereas, It is desirable that all nominations for appointments and promotions of members of the teaching staff at the university, and all recommendations for dismissals, should be made by or through the president of the university, the board of trustees taking no initiative in these matters, and

Whereas, It is undesirable that either the power of appointment or removal should rest absolutely in the hands of a single person,

It is Resolved and agreed by the board of trustees and president of the Leland Stanford Junior University that so long as nominations for appointments and promotions of members of the teaching staff of the university are made by or through the president of the university, no dismissal shall be made without the concurrence of a majority of the trustees present at a meeting of the board of trustees at which a quorum shall be present.

That in the case of the recommendation of the removal of a member of the teaching staff involving any questions affecting his honor or moral character, he shall be furnished by the president, upon application, with a specific written statement of all charges and evidence reflecting upon his honor or moral character, and be given an opportunity to present a written statement of his answer and of any evidence he may wish to offer in defense, and a copy of all such charges and evidence, together with any answer and evidence

[1] Concluding part of the annual report of President David Starr Jordan.

offered by the accused, and the recommendations of the advisory board shall be attached to the recommendation of the president of the university, and the action of the board of trustees shall be based solely upon the recommendation of the president of the university and the record attached thereto, there being no further hearing before the board of trustees or any member thereof, unless the board in its discretion shall elect to receive other evidence in aid of its decision, and any such recommendation and information affecting the honor or character of a member of the teaching staff shall be presented to and acted upon by the board of trustees separately from anything which may involve his competency or fitness in any other respect. The members of the board shall not in any case, or in any event, listen to or receive any statement concerning such matter except in open meeting.

SCIENTIFIC NOTES AND NEWS.

PROFESSOR IVAN PETROVITCH PAVLOFF, professor of physiology in the University of St. Petersburg, will deliver the Huxley lecture at the Charing-cross Hospital Medical School on October 1.

THE annual address before the Sigma Xi Society of the University of California was given on September 4 by Professor H. S. Jennings, of Johns Hopkins University, on the subject 'The Behavior of Some Animals of the Seashore.'

DR. S. KIMURA, professor in the Imperial Japanese Navy, is passing through this country on his way to Berlin, Germany, where he will attend the wireless telegraph conference.

MR. W. MARCONI expected to sail for this country on September 15, on the *Caronia*.

DR. LAWRENCE F. FLICK, director of the Phipp's Institute of Philadelphia, was one of the delegates to the Congress on Tuberculosis which met during the first half of the present month at The Hague.

PROFESSOR D. S. JACOBUS, who has been connected with the Stevens Institute of Technology since 1884 as instructor and professor of experimental engineering, has resigned to join the technical staff of the Babcock and Wilcox Boiler Company.

DR. L. A. BAUER'S resignation from the U. S. Coast and Geodetic Survey took effect on September 1. As already announced in this journal, he has accepted the permanent directorship of the department of terrestrial magnetism of the Carnegie Institution of Washington. All his correspondence should be addressed to The Ontario, Washington, D. C.

DR. HARRY T. MARSHALL, formerly of the Johns Hopkins University, has been appointed pathologist at the Bureau of Science at Manila.

DR. J. STEIN, S.J., has been appointed astronomer in the Vatican Observatory at Rome.

ACCORDING to the *Journal of the American Medical Association*, it is rumored that Professors von Leyden and Olshausen intend to retire from the medical faculty at Berlin University. Professor E. v. Bergmann will also give up further teaching this fall when he passes his seventieth birthday, and Professor W. Erb, of Heidelberg, will retire at the close of the winter semester.

DR. ELMER E. BROWN, recently appointed U. S. commissioner of education, will give the opening address at the exercises of the School of Pedagogy of New York University, on September 22.

AT the recent Boston meeting of the American Academy of Medicine, Dr. Casey A. Wood, of Chicago, was elected president. Dr. F. Trendelenburg, professor of surgery at Leipzig, who was in attendance at the meeting, was elected an honorary member.

THE Simon Fund of $25,000 for the furtherance of research on syphilis has been divided between Professor Neisser, of Breslau, who receives $19,000; Dr. J. Siegel, who receives $4,500, and Dr. Lesser, to whom $1,500 has been awarded.

HERR O. WENTZKI, of Frankfurt a. M., has been awarded the 300-marks prize of the Berufsgenossenschaft der chemischen Industrie for the discovery of the best means of purifying hydrogen which contains arsenic.

AT the meeting of the Association of Military Surgeons, held in Buffalo last week, it was announced that the Enno Sanders prize had been awarded to Major Pilcher for an

essay on 'The training of the medical officer of the state forces to best qualify him for local service and for mobilization with national troops.'

Professor Chas. N. Gould is preparing a preliminary report on the oil, gas and coal deposits of the new state of Oklahoma. The field work was accomplished during July and August.

The Journal of the American Medical Association states that a memorial tablet was recently unveiled at Pavia, Italy, to the memory of the late surgeon, E. Bottini. Behind the tablet were immured a gold medal with the portrait of the deceased and an elaborate souvenir volume containing works by his pupils and friends, with an address on parchment—all of which had been prepared to present to the master on the occasion of the twenty-fifth anniversary of his assuming the chair of surgery at Pavia. The plans for the ceremonies were annulled by the unexpected death of the great surgeon not long before the arrival of the anniversary. The tablet stands between one erected to the memory of Scarpa and another to Porta, and the inscription on the marble is merely a few words of tribute to the man 'who by his genius in the science and the art renewed the glorious traditions of the Pavia school of surgery.'

M. Joseph François Bossert, astronomer at the Paris Observatory, has died at the age of fifty-four years.

Dr. Max von Eyth, known as an engineer and author, died on August 21 at Ulm, at the age of seventy years.

Dr. John Cameron, consulting physician to the Royal Southern Hospital, and formerly lecturer on medicine at the Liverpool Royal Infirmary, has died at the age of eighty-eight.

There will be on October 17 a civil service examination for the position of scientific assistant in the Department of Agriculture, at salaries ranging from $840 to $1,200 per annum, depending upon the qualifications and experience of the appointees. Attention is invited to the fact that it is especially desired to secure application for the major optional subjects—analytical chemistry (methods for the detection of food adulteration), plant pathology and soil physics.

The Illinois Civil Service Commission announces examinations for a pathologist at Kankakee ($2,500 a year and maintenance), and pathologists in six other hospitals for the insane.

The Mexican government offers three prizes, each of the value of $20,000, for (1) the discovery of the typhus fever germ; (2) the mode of its transmission to man; (3) a successful preventive or curative serum or other effectual remedy.

Professor A. D. F. Hamlin, of Columbia University, has been authorized to offer a prize of $500 in behalf of the American Society for the Prevention of Cruelty to Animals for the best design for a bronze drinking fountain for dogs, horses and men. It is to be set on the edge of the sidewalk. Each fountain is to be limited in cost to $250.

An association for the study of microbiology has recently held its first session at Berlin.

The first international congress for the study of the polar regions opened on September 7 at the Palais des Académies, Brussels, under the presidency of M. Beernaert. As the London *Times* states, the idea of the systematic study of this portion of the globe originated in the international congress on 'expansion mondiale' held last year at Mons, when a committee was formed to carry the idea into practise on a basis of international cooperation. At the meeting representatives were present from the institutions and learned societies of almost every quarter of the globe. Among those present were Dr. Nordenskjöld, M. Arctowski, M. de Gerlache, Captain Scott, Prince Buonaparte, and many leading men of science. Baron de Favereau, the Belgian minister for foreign affairs, welcomed the delegates. It was decided to form a committee to study the best means of creating an international polar association for the purpose of organizing explorations and otherwise assisting the object in view. Dr. Jean Charcot announced that he was organizing an expedi-

tion to the South Pole and Charles Benard, president of the Belgian Oceanographic Society, said that society was organizing an expedition to the North Pole.

The British Medical Journal says in an editorial article: "In regard to yellow fever, Panama affords as striking an object lesson as Havana of the incalculable benefit to mankind that has followed the discovery of the cause of the disease and the manner of its transmission. The glory of the work which has had this striking consummation is shared by several men. The credit of the conception belongs to Dr. Carlos Finlay, who propounded the idea many years ago without attracting from the profession any attention but an occasional contemptuous notice. More fortunate than many true begetters of new truths, Dr. Finlay, at the meeting of the Pan-American Medical Congress held at Havana in 1901, was acclaimed by the assembly as the author of the discovery which has already been so fruitful of good effects. Dr. Carter was another pioneer in the work which was brought to completion by the American Commission. Untimely death snatched the reward from the hands of Walter Reed and Lazear, but Drs. Carroll and Agromonte still survive. It would, we think, be a fitting acknowledgment of the work of these four men if the Nobel prize were divided among them. It will scarcely be denied by any one conversant with the facts that their work is of far greater importance than that of several to whom the prize has been awarded in the past few years. The only original research work whose practical results can be held to compare with it is that which has brought malaria, that monster which till lately claimed so vast a tribute of human lives, within the control of man."

The Geographical Journal states that a new railway just opened in China traverses a fertile and populous tract of alluvial country in the province of Kiangsu. It runs for a distance of about 200 miles from Shanghai to Nanking in a generally northwesterly direction through the towns of Su-chou, Wu-sieh and Chin-kiang. The section actually opened to traffic, however, is from Shanghai to Su-chou and Wu-sieh, a total distance of 85 miles. The remaining portion to Chin-kiang and Nanking enters into more hilly country, where some tunnelling is necessary, and this, it is anticipated, will not be completed and opened for traffic until 1908. The country round about is one of the most thickly inhabited and productive parts of China. It is intersected with numerous creeks, canals and tributary streams draining into the Su-chou Creek, the Hwang Pu River and the Yang-tse Kiang. Cotton, rice and mulberry trees are grown extensively, and there is a great passenger traffic along the Grand Canal, as well as endless junks freighted with timber, stone, grain, pottery, bamboos and miscellaneous goods. The length of time occupied by passengers in traveling from Shanghai to Nanking will be reduced from about twenty-eight hours to eight or ten hours, and the goods traffic, which now occupies several days, will gain in an even greater degree. Su-chou and Chin-kiang, in addition, of course, to Shanghai and Nanking, the termini of the line, are treaty ports. The first named has been called the Venice of the Far East, owing to the network of canals that pass through its curious narrow streets. It was besieged by Colonel Gordon during the Taiping rebellion, and is now a great center of the silk industry. Wu-sieh and Chin-kiang are also busy marts, the latter, as well as Nanking, being a regular calling place for the steamers that ply on the Yang-tse.

WE learn from the London *Times* that a large collection of South American birds has been presented to the Zoological Society by Captain Pam, who brought them with him from Caracas. At the wish of the donor, Mr. Thomson met the mail boat at Southampton and took charge of the birds, which he conveyed to the gardens without any losses. To Captain Pam belongs the credit of being the first person to introduce any species of the humming-bird family to the Regent's Park aviaries. Last November he presented one of the 'violet ears' (*Petasophora iolata*), the only survivor of the half-dozen with which he started, and the bird lived for a fortnight,

though it suffered badly from the cold during the latter part of the passage, and footwarmers were used to provide artificial heat for it on the train journey. Short as the life of this example was in captivity, it exceeded by some days that of others kept by a private aviculturist in Gray's-inn, who a few years ago purchased eleven, and lost them all within as many days. The humming-birds which arrived on Monday have been placed in the insect-house, one end of which is now occupied by two large glass cases specially prepared for them, and quite large enough to allow of somewhat extended flight. On their passage to England the birds were fed, as they will be at the gardens, on meat extract mixed with honey, the former being intended to make up for the insects which they obtain in a natural condition, but which are difficult to supply in captivity. Even in South America native-born aviculturists reckon themselves successful if they keep a caged humming-bird for a couple of months. This period, however, was doubled by Captain Pam, who had one of these birds for four months. These newcomers have not yet been identified, but it is believed they are closely allied to the violet-eared humming-bird exhibited last November. The insect-house was closed to the public on Monday, but it will soon be open again, and the tropical birds kept there should prove a great attraction. Much more numerous was the other part of the collection, which has been deposited for the present in one of the inner rooms of the western aviary. The only one that has yet been turned out of the traveling cage is a sun-bittern, none the worse for its journey. The other species, of which there were in all over eighty examples, consisted of maroon and blue-shouldered tanagers, golden-crowned harynests, mocking birds, a small woodpecker (new to the collection), hooded siskins, buntings and finches.

UNIVERSITY AND EDUCATIONAL NEWS.

By the will of Edward H. Dunn, president of the corporation of Boston University, $120,000 is left to that institution, one half for the establishment of a professorship and one half for general purposes.

We learn from the *Medical Record* that the Medical College building in connection with Queens University, Kingston, was destroyed by fire recently. Although there was an insurance of only $22,000 steps were immediately taken to erect a new building, which it is expected will be ready for use early in October. The fire, it is thought, originated from a gas jet kept burning in an oven where paraffine molds were made. The destruction of the bacteriological and public health laboratories in connection with the college will for the present cause considerable inconvenience.

At the last meeting of the regents of the University of Nebraska, Dr. F. E. Clements was promoted from the associate professorship of plant physiology to the full professorship.

Ensign C. A. Harrington, U.S.N., who graduated from the Naval Academy two years ago, has been assigned to give a course in naval architecture at the Massachusetts Institute of Technology.

Mr. Frank F. Grout, A.B. ('04, Minnesota), has resigned from the Illinois Geological Survey to accept a position as instructor in mineralogy at Oklahoma State University.

Appointments in philosophy and psychology are announced as follows: Mr. Rowland Haynes, assistant in philosophy at Columbia University during the winter of 1905–6, has been appointed associate in philosophy at the University of Chicago for the ensuing year. He is to have courses in philosophy, ethics, psychology and logic during the absence in Europe of Associate Professor A. W. Moore. Dr. Harvey Carr, of the University of Chicago, has been appointed instructor in psychology in the Pratt Institute of Brooklyn, to succeed Dr. Irving King, who goes to the University of Michigan. Dr. W. K. Wright, of the University of Chicago, has been appointed instructor in philosophy and psychology in the University of Texas, to succeed Dr. Warner Fite, who has accepted a call to the University of Indiana. Dr. Percy Hughes, instructor in philosophy at the University of Minnesota, will give the courses at Tulane University during Professor W. B. Smith's absence in Europe.